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RESEARCH IN LANGUAGE ARTS.

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VERY LITTLE IMPORTANT RESEARCH HAS BEEN DONE IN THE FIELD OF LANGUAGE ARTS, AND THE RESEARCH INTEREST IN THAT FIELD ENDS WITH THE PUBLICATION OF RESEARCH REPORTS WHICH OFFER PRONOUNCEMENTS RATHER THAN RESEARCH EVIDENCE. THE PURPOSES OF RESEARCH OR HOW ITS FINDINGS COULD BE USED HAVE NOT BEEN CLARIFIED. THERE IS ALSO BLIND ACCEPTANCE OF WHAT AUTHORITIES SAY. SOME MAJOR PROBLEMS IN HANDWRITING, SPELLING, LISTENING, ORAL EXPRESSION, AND IN WRITTEN EXPRESSION NEED INVESTIGATION. MORE THOROUGH EXAMINATION OF PROPOSALS ADVANCED, OF THE FINDINGS OF LANGUAGE SCHOLARS, AND OF THE DIRECTION BEING TAKEN BY CURRICULUM CHANGE SHOULD BE MADE. THE HELP OF THE NATIONAL COUNCIL ON RESEARCH IN ENGLISH AND OTHER ORGANIZATIONS IS NEEDED TO RELATE CURRENT TEACHING MATERIALS AND TECHNIQUES TO THE BEST AVAILABLE RESEARCH EVIDENCE AND TO DISTRIBUTE LISTS OF SPECIFIC PROBLEMS TO AGENCIES INTERESTED IN FUNDING EDUCATIONAL RESEARCH. THIS PAPER WAS PRESENTED AT THE NATIONAL CONFERENCE ON RESEARCH IN ENGLISH (NEW YORK, FEBRUARY 17-18,1967). (NS)

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Research in Language Arts

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The invitation to talk to this group about needed research in language arts, to state a position regarding research in this area, caused me to pause and reflect upon the advisability of accepting it. Certainly there is considerable professional danger for anyone to state his views concerning research needed, since the subject itself implies criticism of what has occurred. Further, such a statement provides opportunity for exposing one's ignorance, lack of perception, and biases. In addition, I have other misgivings. It seems to me that increasingly statements of research needs are a popular way to give an organization a research tint that isn't maintained. Too often little action results, particularly any that is significant. I hesitate to be a part of such an activity.

I fully considered these dangers and misgivings, but concluded that I had probably already exposed my ignorance and biases to audiences during the past several years. Further exposure could surely do little harm, and that while my statement of research needed may not result in the follow-up I hope for, it might provoke some thought about the problems.

These conclusions may prove to be unjustified, but my interest in the topic and the assurance of a captive audience overcame my qualms.

My interest in this assignment stems primarily from reviewing reported research over the past five years, reflecting upon its quality, emphasis, and sources--and both appreciating it and dispairing about it.

RE 000 983

Added to this are similar reflections arising from the preparation of articles for the NCRE bulletins on handwriting and spelling and oral language, and, most recently, the examination of studies on the teaching of vocabulary.

Many of us would surely agree that educational research is in a period of revival. The evidence for this abounds. For instance, the AERA program this year has over 600 participants, while five years ago there were fewer than 200. Universities have established centers for training educational researchers. Many conferences concerned with educational research have been held. New journals for reporting research have been founded. Prizes have been given for research done. Even the classroom teacher has been encouraged to "experiment," and one hears curriculum supervisors referring to their "pilot studies" and "experimental programs."

There are several reasons for this research revival. Certainly the increasing interest in science is one. But the principal factor has undoubtedly been the greater availability of funds, with the U.S. Office of Education providing most of these, though with foundations and organizations--including the modest effort of the National Council of Teachers of English--also contributing.

In addition, there has been some impetus from and increasing recognition by the profession that we have too long been upon a research plateau and that a surge ahead is needed.

Not everything concerning the research revival has been or is positively regarded, however. Faults have been found with the research effort, with its quality, and with the issues and problems with which it has dealt.

For instance, while the impetus given to research by federal funding was at first viewed by many as the mañanna that had been sought, doubts are beginning to arise concerning its value and the impact it is having. The February issue of the Educational Researcher[1] points out that the University of Illinois College of Education faculty has suggested that the federal money has too often been spent for the promotion of educational fads.

In this same publication, the president of AERA, Julian Stanley, is quoted as saying to a House of Representatives subcommittee, "I seem to see too much tendency in USOE to 'call the shots' and restrict funding of volunteered proposals to what appear to be pet topics of certain USOE staff members."[2]

And more directly related to our English language arts interest, is Albert Kitzhaber's remark in the October, 1966, Elementary English that many an applicant requesting funding of research has ". . . declined to tailor his proposal to USOE specifications, or . . . has finally rebelled at the delays, the mixed signals, the red tape. . ."[3]

Possibly these thoughts, in general, are shared by you who have dealt with the Office of Education. I share them to a certain extent. However, I suspect that the basis for the criticism by the Illinois group and Mr. Stanley is different from that of Mr. Kitzhaber. Probably the U.S. Office is "catching it" from both, or perhaps several sides, with some of the criticism deserved and some undeserved.

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[1]Jason Millman (ed.), Educational Researcher, Vol. XVIIL, No. 1, February, 1967, p. 5.

[2]Ibid., p. 9.

[3]Albert Kitzhaber, "A Time of Change." Elementary English, October, 1966, p. 626.

Beyond these criticisms, which may be more a result of the lack of concerted leadership within the profession than anything else (a position I intend to refer to shortly), dissatisfaction with educational research has been expressed in other ways.

For example, in 1961, John Carroll, writing in the Phi Delta Kappan, said "There has been too much retreat to measurement and methodological studies without a corresponding sign of progression to the kinds of descriptive and experimental studies which are at the real cutting edge of our science."[4] And he continued with, "I suspect that we have come to honor methodological rigor and finesse more than we honor substantiality and relevance of results."

More recently--in fact in the issue of the Educational Researcher referred to--Richard Dershimer stated, "The bulk of the research in education probably has not been relevant to the major issues facing schools today. . . "[5]

As true as these statements may be, many of us concerned with teaching and research in the English language arts feel that some substantial research strides have been made in this area of the curriculum. We point with considerable satisfaction to the studies by Loban, Strickland, Hunt, and Braddock, and to some of the First Grade Studies. At the same time we know that great gaps in the knowledge we should have remain. Most of these have not received attention, and it appears that there is little effort being made to remedy this condition.

A particularly interesting criticism of research in the language arts was published recently. In an article entitled "English Teaching Encounters Science," in the December issue of College English, George Henry reviews the efforts to bring English and the "scientific approach" together, calling attention to the many conferences of recent years and the products emanating from them.[6] He discusses the emphasis upon research evident in the publications Basic Issues in the Teaching of English, Needed Research in the Teaching of English, The Allerton Park Conference on Research in the Teaching of English, Research Development Seminar in the Teaching of English, and Research Design and the Teaching of English, and finds fault with most of it. Henry's purpose is to suggest that English is attaching itself to an archaic science, one that focuses upon "discrete, packaged, 'rigorous' studies unrelated to a large context or comprehensive frame . . ."[7] echoing Carroll's statement referred to earlier.

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[4]John B. Carroll, "Neglected Areas in Educational Research," Phi Delta Kappan, May, 1961. p. 340.

[5]Op. cit. p. 8.

[6]George H. Henry, "English Teaching Encounters Science." College English, December, 1966.

[7]The Basic Issues in the Teaching of English (pamphlet).Champaign, Illinois: National Council of Teachers of English, 1959, pp.16. Needed Research in the Teaching of English, Proceedings of a Project English Research Conference, May 5-7, 1962, prepared by Erwin R. Steinberg, Washington: U.S. Department of Health, Education and Welfare, OE-30010, Cooperative Research Monograph No.11, p. 134. Proceedings of The Allerton Park Conference on Research in the Teaching of English, Dec. 2-4, 1962, (Robert W. Rogers, Seminar Director). Report prepared by Richard Wasson. The U.S. Office of Education in Cooperation with the University of Illinois, Project No. G-1006, Contract No. OE 3-10-058, p. 126.
Research Development Seminar in the Teaching of English, Feb. 27-Mar. 2, 1963. Report prepared by Louise M. Rosenblatt, Seminar Director. U.S. Office of Education, Project No. G-009, p. 79.
Research Design and the Teaching of English, proceedings of the San Francisco Conference. David H. Russell, Director. Champaign, Illinois: National Council of Teachers of English, 1964. p. 151.

In addition, Henry suggests that the conferences have been concerned with ferreting out what research is needed and have spent too little effort to "determine the riverbed or large contexts in which it operates."

I am not as concerned as Mr. Henry about the attachment of research in English to packaged and rigorous studies; I am not as optimistic as he is in believing that this is true. In fact, to me there seems too little such attachment.

In spite of the conferences, the publications, the curriculum centers, and the studies, I simply have seen very little important research. The research interest of the conferences has generally ended with the publication of the report. And while some research effort that we applaud has been rigorous, well designed and executed, many of you would surely agree that much of it has not.

Even the curriculum centers, the special pride of some persons, have largely made pronouncements rather than presenting evidence of defensible research.

I can agree that, of the research done, there has been too much examination of "minor" questions and too little seeking of answers to "big" ones, with the best evidence of this in the curriculum reform efforts but also with it present in specific studies that could genuinely be called research.

In line with Mr. Henry's criticism, I recall that after viewing a demonstration in Washington last year in which a group of elementary school children were being taught to locate and see the relationship of determiners to nouns, a member of the audience asked the purpose of the lesson. The question was not answered, yet this demonatration came from a program, a "try-out," that some would describe as research.

It seems to me that this was--and is--a basic philosophical question, one that goes right to the "riverbed" in its import. The questioner was asking, "Why are you having children do this? What is it teaching them, and for what purpose?"

This leads me to my topic--needed research--though by no means do I intend to set about stating all that I can think of that should be done.

I do, first, want to consider a major issue--related to what I have just said. It is not just a single research problem. It could be divided into many worthy areas, each deserving of research attention. It is primarily concerned with philosophy, the purpose of our efforts, the "large context in which (the English language arts) operates."

Most of us have been acutely aware for the past ten years or so of the rise of the discipline-centered attempts at school curriculum reform. For instance, we have seen schools throughout the nation adopt the products of the Physical Science Study Committee, the University of Illinois Committee on School Mathematics, the School Mathematics Study Group, and others, in what has seemed to many of us a largely uncritical and non-scientific fashion. Certainly little evidence was advanced concerning these products other than that they soundly

presented the content of the discipline, and had been taught to children. In the wave of post-Sputnik fear and guilt feelings about the neglect of education, no other evidence was sought.

Only recently have I heard of research directed at testing the value of these materials and the methods suggested by them.

A few days ago I heard James Ryan of the Minnesota National Laboratory state that his examination of the attitudes toward and interests in mathematics of a large sampling of ninth grade pupils using two of the widely used "new math" programs showed no advantage for them, and, in fact, that the results favored the pupils taught the traditional program.[8]

I suspect few people in mathematics teaching would favor a complete return to a pre-1951 approach, because of this report, since there are surely some values to the new math, but it seems to me particularly unfortunate that in a curriculum area that is much more "scientifically based" than the language arts this kind of research has been so long in coming, especially when proposing and selling were done so promptly.

Do you see a similar pattern on the language arts horizon? I do. In fact, it is much closer than the horizon.

Should we not, then, be more thoroughly examining the proposals being advanced, the curriculum directions being taken? Perhaps we are, but my impression is that questioning is regarded as out-moded, or even evidence of ignorance.

The fact remains, however, that questions do need asking and answering.

Should we accept--without question--the linguist's information, and the implication of some of them that this knowledge should be transmitted to children? Are the products of the discipline-centered curriculum centers automatically of value? Is the content of their products what children should be taught? What is accomplished from such teaching? How do we know?

In my opinion we are in the process of accepting a curriculum content and the methods of instruction related to it from a basis of authority rather than research. And it is an authority that ignores a great deal about society and about how learning occurs. Further, we are giving sanctity to our act by accepting "try-outs" and the preparation of materials and plans as research.

Please note that I have not said that curriculum reform is not needed, or that the language scholars' findings are not extremely important.

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[8]School of Education Colloquium, State University of New York at Buffalo, February 6, 1967.

The fact remains, however, that there has been very little research related to the "reform" or how the findings should be used. I simply do not view the "New English" movement in its application to schools as research oriented.

None of us should be against cooperation; neither should we be against interdisciplinary action. We actually need more cooperation, more concerted action--but it needs to be genuinely cooperative. The cooperation I have seen has too often been one way, if not by intent, at least in reality. Perhaps, though, the chief reason for this is the abdication of leadership by the persons historically most concerned with language arts teaching and research. We have been so awed by the language scholars' findings and the jargon they use that our good judgment has been circumvented and the knowledge we have from earlier research has been ignored.

I am leaving this issue at this point for two reasons. First, while this position is subject to elaboration, I do not wish to take that advantage of your captivity. Those of you who might agree with me can undoubtedly think of many of the research problems related to the position. Second, I want to state some specific areas and problems for research that are not directly related to this position but which seem to me to be important. These will be limited in number.

First, the gaps in handwriting research have been known for many years. West in the 1950 edition of the Encyclopedia of Educational Research said, "The lack of constructive basic research in the field of handwriting still continues."[9] Harris virtually echoed this in 1960, commenting specifically with respect to handwriting quality that "surprisingly little basic research has been done to rigorously define and analyze the qualities . . ."[10]

In 1962, Ernest Horn concluded a provocative statement on questions for research on handwriting by stating "The importance of handwriting and the universality of the problems warrant support of basic research as well as research on practical problems of immediate concern."[11]

And even more recently, Andersen suggested that while much has been reported about handwriting the overwhelming majority of this writing has not been the reporting of research.[12]

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[9] Paul V. West, "Handwriting," in Walter S. Monroe (ed.), Encyclopedia of Educational Research, Rev. Ed. Macmillan (1950), 524-529.

[10] Theodore Harris, "Handwriting," in Chester Harris (ed.) Encyclopedia of Educational Research, 3rd edition. New York: Macmillan Co., 1960.

[11] Ernest Horn, "Questions for Research on Handwriting," The Elementary School Journal, March, 1962, pp. 305-312.

[12] Dan W. Andersen, "Handwriting Research: Movement and Quality," Elementary English, January, 1965, p. 46.

Of the many problems in handwriting needing research attention, Horn in his 1962 article described five of a major nature, with 21 sets of specific questions. I'll not review these, but I think they have not been researched.

Let me suggest, though, just two major problems in this area:

1. Should we continue to spend time teaching children both manuscript and cursive writing? Or, if we consider the fact that most adults have modified the handwriting forms they learned in school so that they write neither, what form of handwriting should be taught? Isn't there reason to believe that a form or system of handwriting can be identified that is better than either manuscript or cursive with respect to its readability, the speed with which it can be written, the ease which it can be learned, and with the resulting time gained from teaching only one set of letters and movements devoted to other things?

2. How should handwriting be taught for the instruction to have carry-over to all of a child's writing activities? What evidence do we really have that any particular training or materials we provide to teachers is more valuable than some other?

Looking at commercial materials we see little agreement about programs, methods, or anything else.

Turning next to spelling, it seems to me that there are fewer research gaps, though clearly there are two fundamental problems:

1. What are the reasons for the resistance shown by teachers and textbook authors in accepting the evidence favoring such proven instructional practices as the pretest, pupil self-correction of his tests, and the systematic impression-recall study procedures? How can this resistance be overcome?

2. Can pupils be taught the phoneme-grapheme relationships present in our language in a manner that will cause them to spell more correctly than they do when these relationships are not emphasized? And relatedly, can this instruction be given efficiently; that is in a way that is economical of both teacher and pupil time? Too, what are the specific procedures that should be followed in teaching children to make use of these relationships?

In the area of oral expression a great deal could be said about the research needed. However, let me just state two questions:

What actually constitutes "acceptable" or standard speech? Can it be described, and can the phonological, syntactical, or lexical deviations which really interfere with communication or limit the social and educational aspirations of a speaker be determined?

Following this, can we identify the parts of this "acceptable" speech and the sequence in which they are learned in an ideal environment? What then would be the implications for school instruction?

Research in listening is also wide open. In particular, though, let me suggest these questions which might receive attention:

How might the teaching of listening be more effectively incorporated into the school curriculum?

Should certain skills be taught in direct lessons, and what should be the fundamentals of these lessons?

What are the effects of certain conditions upon a pupil's ability to listen? For instance, the physical environment, the personality and emotional state of the listener, the appeal of the material, habits of the listener, how the listener thinks, and so on.

In written expression, the Braddock study presented twenty-four questions under the heading "Unexplored Territory."[13] Combining several of these and extending them somewhat, I suggest these two problems:

1. What are the elements of effective writing? To what extent can these elements be identified, and how are they different for different forms of written discourse?

2. In what ways are abilities in the other language arts related to ability in writing? Will greater attention to oral composition result in better written composition? Will fostering children's interest in language result in better composition?

I have not presented research needed in several other areas of the language arts, nor have I identified more than a few of the problems in the areas I did mention. My principal intent has been to suggest that we need to give further thought to the direction that is being taken in curriculum change, that we seek to gain greater control over this direction--doing so cooperatively with others who are also interested in the content of the curriculum and in the children--and that this direction be determined by research rather than authority. I have indicated that the research that should be done is virtually unlimited and includes both major hypotheses that need testing and specific questions that need answering.

In conclusion, I urge that we now take direct and cooperative action, that this action be much greater and more specific than it has been taken in the past.

I propose that this organization, which by its name should be more concerned than any other with research in English, extend its leadership role by doing the following:

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[13]Richard Braddock, Richard Lloyd-Jones, and Lowell Schoer, Research in Written Composition. Champaign, Illinois: National Council of Teachers of English, p. 118.

1. Draft and adopt a resolution addressed to the teaching profession of this country stating the relationship of the teaching materials and practices now being used in the English language arts to the best research evidence we now have available. Such a resolution should also include the relationship of this research evidence to as many of the materials and practices which are now being advocated as "new," "innovative," and of a "reform" nature as is possible.

2. Draft and adopt a statement proposing the direction of research effort in the English language arts, including in this statement a listing in hierchical form the specific research that should be done. I propose that this statement be directed to the U.S. Office of Education but that it should also be distributed to private foundations interested in educational research and to departments and individuals in universities and colleges.

3. Seek the support of other organizations concerned with the teaching of English in all of its aspects and at all educational levels in support of the preceding statements and the professional and realistic approach to the major problems suggested by them. I see this as an implementation in our field of the "map and communicate" need suggested by Benjamin Bloom in his AERA address of last year.[14]

4. Enlist the support of as many members of NCRE as possible, and their respective institutions, for an immediate and truly cooperative research effort upon a limited number of significant research problems. Acceptance of this requires that the cooperation begin with the research design and continue through the reporting of results. The cooperation should incorporate the best thinking possible in order for genuinely useful information to be gained. This kind of cooperation is essential if we are to avoid the faults in research design and implementation and the considerable amount of misdirected drawing of conclusions that have plagued us.

I believe the immediate effort should be made without seeking federal funds in order to show that this kind of cooperation is possible and that the leadership lies within the profession and not with the source of funds.

I see no valid reason why these proposals could not be accepted and implemented. We might have to sacrifice some things we're now doing. We might have to spend some of this organization's money--or even our own. But the people in this room could do these things. I suspect that if we don't do them some other person will be listing much of the same needed research ten years hence.

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[14]Presidential address presented to the American Educational Research Association, Chicago, February, 1966.